Subject: resistance to scientific discovery
To: Juan.Campanario@uah.es
From: lederberg@mail.rockefeller.edu
Fcc: TXT, /jm/BIOG

Sun Feb 8 15:17:19 EST 2004

Dear Prof. Campanario:

I was interested to get your letter dated 15 Jan '04.
I have been interested in this topic at least since Bernard Barber's
paper (perhaps earlier statements on his part):

AU BARBER, B
TI RESISTANCE BY SCIENTISTS TO SCIENTIFIC DISCOVERY - THIS SOURCE
OF RESISTANCE HAS YET TO BE GIVEN SCRUTINY ACCORDED RELIGIOUS
AND IDEOLOGICAL SOURCES
SO SCIENCE
VL 134: 596 - & ; 1961

This enjoys 173 citations on the Science Citation index; so there is indeed
a venerable history, in sociological discussion. For some scientometric
approaches to assessing resistance and delayed recognition, you surely
must engage: (and visit his home page)

Eugene Garfield, PhD.
Chairman Emeritus, ISI www.isinet.com
Publisher, The Scientist www.the-scientist.com
email: garfield@codex.cis.upenn.edu
home page: www.eugenegarfield.org

The related theme: "premature discovery" has been ventilated at length
by Gunther Stent, see the Festschrift:

Ernest B. Hook, Editor
Prematurity in Scientific Discovery
On Resistance and Neglect
Publication Date: October 2002

And you can search the web under the rubric and find 476 hits.

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My own sociological research has been on postmature discovery. I send
you some reprints bearing thereon
See also:
http://profiles.NLM.nih.gov/BB/A/B/P/N/

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The outstanding examples of (alleged) resistance in biology are

Mendel 1865 -- buried until 1900
Avery et al 1944 -- genes as DNA -- contentious until about 1953
McClintock 1951 -- jumping genes.

I accept the Mendel "story" There is a large literature on it.

I have extensive debate with Stent about the Avery "story"
N.B.
http://profiles.NLM.nih.gov/CC

McClintock is more complicated; Nat Comfort has gone into some detail in his book:
Nathaniel Comfort
Barbara McClintock's Search for the Patterns of Genetic Control

No one doubted her data; but her interpretations had nuggets of brilliance too easily overshadowed by unproductive specualation.

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I believe resistance is indispensable, though scientists are often too quick to dismiss or accept problematical work on a black/white dichotomy. Peer review works pretty well. It's unimportant that journals like Science and Nature are sometimes arbitrary in what they accept, as long as innovative work can be pubished somewhere. The same principle should also govern funding: appeal to multiple founts, and some way to discount a veto by a single skeptic. The internalized dampening of ambition is probably the worst consequence of failings in peer review. I agree with what you write about persistence.

As to my own experience:

1. I've encountered reasonable skepticism, but nothing daunting. Cf. my disputation with Andre Lwoff (1946) -- I send you reprint. I've been turned down by Nature/Science on relatively uniumportant matters, but never by a professional specialty journal. Having won a Nobel Prize at age 33 (12 years after I did the work) I can scarcely complain about delayed recognition. My work had the advantage it could be easily repeated in other labs, and it was. Worse to be ignored! than to be disputed!

As to 3.1: see ellisonfoundation.org to see how that private funding agency tries to encourage innovative ideas. Basically it's another appeal body, committed to seeking diamonds in the dust and willing to take risks.

After you've received my reprints, let me know if you have more questions.

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